Information Science at the University of California at Berkeley in the 1960s: A Memoir of Student Days

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Abstract

The author's experiences as a master's and doctoral student at the University of California at Berkeley School of Library and Information Studies during a formative period in the history of information science, 1966–71, are described. The relationship between documentation and information science as experienced in that program is discussed, as well as the various influences, both social and intellectual, that shaped the author's understanding of information science at that time.

Introduction

I am writing this article not to claim myself as a pioneer of information science but rather to describe what it was like to be a student in the pioneering days of information science. There is much discussion nowadays of the history of information science, and in some instances it is argued that the early twentieth-century documentation theories of Paul Otlet (1990) and Suzanne Briet (n.d./1951) were the intellectual antecedents of information science. I am not a historian of the field, and I make no claim one way or the other about its historical roots. The understanding I developed of information science as a doctoral student in the 1960s at the University of California at Berkeley (U.C. Berkeley), however, had little to do with Otlet, Briet, or documentation in general. We saw information science as something brand new that was drawing on a range of earlier ideas, to be sure, but those sources were from realms very different from documentation. I believe that some of these sources are being lost sight of in the current discussion of the history of information science. In what follows, I present a memoir of my experience as a student at a formative moment

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in information science and describe the effort, as I saw it, to develop the field as a meaningful, distinctive discipline in one large doctoral program in a major university.

Becoming a Master in Library Science Student

Sometimes in life we fall into where we were meant to be all along. Upon returning from Peace Corps service in Thailand, where I had taught English as a foreign language in two Thai high schools, I was confronted with the same question I had had as a fresh college graduate before I left: What should I do with the rest of my life? While sorting this out, I went to live with my parents in Lafayette, California, just over the hills from the University of California at Berkeley. I went down to Berkeley to take aptitude tests and get career counseling. It must be remembered that this was 1965, and women did not routinely get career guidance. Most female life-models for me in those years were homemakers. In fact, despite having attended Pomona College, one of the top liberal arts colleges in the country, I did not personally know a female Ph.D. in a tenure-track position until a high school girlfriend got her doctorate and an academic position.

The woman counselor was blunt: With my Phi Beta Kappa key and B.A. from a good college, about all that was available to me was to "type or teach." In those days, that meant working as a secretary or teaching in elementary or high school. In fact, at that time, the University of California required its secretaries to have bachelor's degrees. Other, more remunerative jobs for B.A.'s at the university went to men. The counselor said I would have to do graduate work of some kind if I wanted an interesting job of any other type. The high school children I had taught in Thailand had been far better behaved than typical American high school students, yet I disliked the little disciplining I had had to do there. I knew I did not want to teach in U.S. high schools.

I sensed that this was the first straight talk I had ever heard about careers and knew that she was right. The trouble was that I did not feel like going back to school. My undergraduate schooling had been very intense, and I wanted to play for awhile. What was worse, in the aptitude tests I scored right down the middle on everything—interested in everything and nothing. What was I to do? I went to the career information center and looked through the brochures for graduate programs. I looked for the program with the shortest time to attain a degree—maybe if the schooling did not last too long, I could stand it. The library program at Berkeley took just one calendar year. I applied.

The admissions officer at the library school asked me if I had ever read anything by Theodore Dreiser. I had not, which worried me a bit, but I was admitted anyway. I needed money, however, as the arrangement with my parents had always been that they would support me through college but not beyond. I applied at the newly founded Institute for Library Research

(ILR), which was associated with the library school, and was hired as a graduate assistant by Ralph Shoffner.

In the first semester in the master in library science (M.L.S.) program, I studied book selection, cataloging, reference services, and the history of the book. Leroy Merritt, who later founded the short-lived library program at the University of Oregon in Eugene, taught book selection with a strong academic library orientation. I became expert at consulting book auction catalogs for out-of-print books. Roger Levinson, a fine printer by trade, conveyed his deep love of books as physical objects as he expounded on them in class in the Rare Book Room of the library.

The library school had its quarters on the top floor of the main Doe Library. Desks for students ran in alcoves next to the windows around the outer wall of the floor. The main library was walled off from the student quarters, and the library for the school was carved out of a portion of the main library stacks. (Later, the school moved into South Hall, the oldest building on campus, which had been renovated for its occupancy.) I was greatly relieved that, as a graduate student, I had direct access to the general stacks of the main library; undergraduates and visitors were not allowed in the stacks and had to handwrite request cards for every book they wanted to examine.

The atmosphere at the ILR made an interesting contrast with the more humanities-oriented world of the library program. The ILR was housed on the second floor of an old "temporary" building on campus that was supposed to be torn down at the end of World War II. The building was painted a pale institutional green and looked like those quickly constructed wooden military units seen in World War II movies. The ILR was directed by Robert Hayes at the University of California at Los Angeles (UCLA), but Ralph Shoffner ran the institute on a day-to-day basis at Berkeley; I reported to him or to others he directed. Shoffner has long since gone on to found his own consulting firm in Oregon, but at the time he was not many years from a very intense engineering education with an emphasis on operations research at the Massachusetts Institute of Technology (MIT). He had a driven quality, a fierce grin, and a wry sense of humor. He lived and breathed systems analysis, and every one of the ILR's projects was approached in a system-analytic way. Systems analysis itself was not so old then; in fact, one of its pioneers, Wes Churchman, taught at U.C. Berkeley.

Everything I was learning while a graduate assistant was new to me; I was unsure of myself and asked questions till I must have driven Shoffner crazy. Trained in the discursive language of the humanities, I found this new way of thinking utterly different, absorbing, and interesting. In the course of the first year I worked there, this way of thinking literally transformed how my mind worked. Gradually, I realized that I had an aptitude for this particular type of analytical thought. I worked on a project to speed up interlibrary loan processes among the University of California campuses by

using fax machines to communicate between campuses; I also worked on a project to get the contents of the catalog of the California State Library in Sacramento into machine-readable form. In those days, there was no "online"; the machine-readable records were used to produce printed book catalogs. I soon formed a plan to become a library systems analyst with my master's degree.

Computers were still a relatively new phenomenon then. Though most librarians favored their use, there were debates in the library literature about whether computers were a good thing for libraries and, if they were used, what they should be used for. I wrote my own FORTRAN programs to do basic statistical analyses on some data for a small research project at the ILR. Such work would be done with standard statistical programs today, of course. At that time, all computer processing was done by feeding punched cards into big mainframe computers. In fact, because sciatica in my hip made it painful for me to walk during one term, I dropped out of a programming course because I could not walk up the hill over and over again to where the computers were in order to pick up my paper printouts.

My triumph as a student assistant in the ILR came one day after I and two others had been sent in a university car to Sacramento to draw a sample from the State Library's card catalog. I soon realized that the sampling method we were using was seriously biasing the results. Upon our return, it took me forty-five minutes to persuade my supervisor, who was just a rung above me in the institute hierarchy, that what we were doing was not right. He was finally convinced, and we retook the sample.

Going for the Doctorate

One day at the ILR, Shoffner said to me, "So when are you going to apply to the doctoral program?" I had not seriously entertained this thought before but, when asked in this way, it seemed like quite a natural thing to do. At about this time other events took place that also made going on for a doctorate seem like an exciting thing to do. The federal government was dramatically expanding support for doctoral students in library and information science (LIS), in the form of what were known as Title II-B grants. At the same time, the Berkeley program launched a new information science emphasis with the hiring of M. E. (Bill) Maron and, a while later, William Cooper, Victor Rosenberg, and Michael Buckland. The new direction was exciting and felt like a natural follow-up to my interest in library systems analysis. I applied for and was admitted to the program and also received a three-year Title II-B fellowship. Fellow students entering the program within a year or two of my entrance included the following (for those who went on to teach, their university affiliations are given in parentheses): Hilary Burton, Michael Cooper (University of California at Berkeley), Ruth Gordon, Theodora Hodges (University of California at Berkeley), Caryl McAllister, Edmond Mignon (University of Washington),

Jerry Nelson (University of Washington), Barbara Nozick, Ruth Patrick, Ralph Shoffner, Keith Stirling (Brigham Young University), Irene Travis (University of Maryland), Diana Thomas (UCLA), Howard White (Drexel University), and Harriet Zais.

Within weeks of Maron's arrival, I and nine other doctoral students, had signed up to be his advisees. That was, I believe, a majority of the doctoral students in the early stages of the program, even with the boost of the Title II-B grants, and gives an indication of the enthusiasm and excitement surrounding the new initiative. Maron taught a course entitled Introduction to Information Science, which drew many students and, in effect, defined what information science was for the Berkeley program.

I have been unable to find my notes from Maron's class; however, a couple of years later, I was invited to teach the same introductory course as an acting instructor.[1] The introduction in my notes for the class states that I retained the content of Maron's course largely intact. The principal changes were my additions of a section on user studies and some material by Marshall McLuhan. Here is the main sequence of content of the quarter-length course syllabus as I presented it in spring 1970. Each indented line represents one class day; the class met three days a week for one hour.

Librarianship 240, Spring 1970
Introduction to the Information Sciences

I. Introductory
  The Information Explosion
II. The Organization of Information for Access
  What Is "Access"?
  Some Indexing Systems-I
  Some Indexing Systems-II
  The Descriptive Continuum
III. Automatic Procedures
  Set Theory
  Computers-I
  Computers-II
  Artificial Intelligence
  Automatic Indexing and Abstracting-I
  Automatic Indexing and Abstracting-II
  Associative Indexing
  Search Strategy
  Question-Answering Systems
  Field Trip
  Midterm
IV. Analysis and Evaluation
  Systems Analysis (guest speaker)
  "The Scientific Method"-I[2]
  "The Scientific Method"-II
  Statistical Procedures-I
  Statistical Procedures-II
  The User in the System-I

The User in the System-II
The User in the System-III
Evaluating Information Systems-I
Evaluating Information Systems-II
Computers and Privacy
Overflow

For a while, Maron was the only information science faculty member in the program, and the question quickly arose for those of us in the area of what other courses to take to prepare for the field. Maron later developed a follow-up information science course, which I took, and offered a seminar. I took other courses in the library school as well, which I will describe shortly. One person does not a discipline make, however. There was generally a feeling, supported and promoted by Maron, that information science was developing out of a number of disciplines, and a full education in the field required gaining that knowledge from outside the program as well as within. That often meant taking a course only part of which was of interest for my purposes. In the end, with the help of that wonderful fellowship, I took three full years of classes, culminating in qualifying exams in March of 1970.

Partly on Maron's advice, and partly based on my own interests, I took or audited the courses listed below, which were offered outside the library school. (I have made up the titles, as the Berkeley transcripts are quite cryptic.) Home departments for the courses are listed in parentheses; these are quarter, rather than semester-long, courses.

Introduction to statistical inference (Statistics)
Probability theory (Statistics)
Cost/benefit analysis (School of Public Health)
Linear algebra (Mathematics)
Reading course in communication research (Psychology)
Psycholinguistics (Psychology; took one quarter, audited second quarter)
Artificial intelligence seminar on automatic game-playing[3] (Psychology)
Propositional and first-order logic (Philosophy)

In the end, it was the social science work, rather than the mathematical, that most appealed to me, but the math enabled me to understand reasonably well the formulas and theory behind Gerard Salton's work (1968) and Maron's own work (Maron, 1961; Maron & Kuhns, 1960) on the design of automatic indexing systems.[4] One of my two chosen doctoral exam specialization areas was then known as "intellectual access"; it would be called "information retrieval" in most schools today.

In a seminar with Maron, I wrote a lengthy paper analyzing and comparing eleven efforts that had been made to that time to come up with formulas for effective automatic indexing. The paper covered the work of

H. P. Luhn, Don Swanson, Fred Damerau, Harold Borko, and Paul Switzer, among others. It also covered the work of three women: Phyllis Baxendale, Myrna Bernick (Borko's coauthor) and Mary E. Stevens. Stevens wrote a widely cited review of literature on automatic indexing (1965), which I relied on a great deal in my studies. Many of these indexing approaches were re-invented in the 1990s in the early days of Web retrieval. In the 1960s the emphasis was on automatic indexing, rather than automatic retrieval, but the thinking was essentially the same. In another seminar, I wrote about the history and applications of citation indexing.

The Social Context of the Times

Before I discuss other intellectual influences, something should be said about the general context of the times and its impact on this particular student. I started the M.L.S. degree in February of 1966, the last semester before Berkeley switched to the quarter system (they switched back to semesters again after I left). I started the Ph.D. program in the spring quarter of 1967 and left to take a teaching position at the University of Maryland in January of 1972, finishing the doctorate in December 1972. Those years that I was at Berkeley, 1966–71, encompassed most of the years of the 1960s revolution, of which it might fairly be said that Berkeley and San Francisco were the national headquarters. Those years were a time of almost continual ferment—there were movements for black and female equality, for sexual liberalization and general relaxing of rigid social constraints, and in opposition to the Vietnam War.

One cannot understand how liberating the 1960s were without understanding how oppressive the 1950s were for anyone raised during that decade. Our parents' generation, which had had a long hard slog through the 1930s Depression and World War II, just wanted peace and quiet, and they enforced that desire with an imposed conformism that was frightening in its intensity. (I am speaking about society in general here; my parents were not particularly strict.) Young people nowadays who wish they could have lived in that time would, in fact, be horrified at the almost Victorian constriction of 1950s life.

Not surprisingly, the prospect of equal rights for women in the 1960s had particularly intense meaning for me. I read Betty Friedan's *The Feminine Mystique* (1963) early on. When I heard that an organization called the National Organization for Women was being founded, I made the necessary contacts to be involved in the creation of the first West Coast chapter of NOW in San Francisco.[5] It was organized by a middle-aged businesswoman named Inka O'Hanrahan. I was one of the youngest women at the founding meeting, and I am proud that I was the recording secretary for the meeting.

For a time I was part of a conscious-raising group (as they were then called) that we billed as a group for women who had already had their consciousnesses raised. Oh, were we naïve! I believed that once men realized

that they had been discriminating against women, they would be happy to change things to make them more fair. I was baffled when they seemed to be angry that we wanted equal rights. After all, we were the ones who had been discriminated against! It was a long time before I understood that equality for women and for men of color caused some white men to feel they were losing their former privileges.

The pervasive inequality of women in society was certainly reflected in the university as well, including the library school. Despite the fact that about 30 percent of the doctorates in librarianship had gone to women at that time (based on a count I made at the time in Cohen, Denison, & Boehlert, 1963), there were no women in tenure-track positions in the school. (The one exception, Anne Markley, had only a master's degree and had been tenured and promoted to associate professor many years earlier, when that was still possible without a doctorate.) There was, in fact, a kind of upstairs/downstairs culture at the school, with the professors having all the privileges of tenure-track faculty and the lecturers and cataloging revisers constituting the downstairs, with much less pay and security. Most of the latter were women. In fact, this culture was so established, accepted, and out of consciousness that it was not until close to the time I graduated that I finally noticed that the work of cataloging instructors, such as Grete Frugé, was also about "intellectual access," and I wondered for the first time why there was not more connection between her work and Maron's.

Throughout this time, I participated in many marches against the Vietnam War. The movement climaxed in May of 1970, at the time of the U.S. invasion of Cambodia, which seemed a particularly egregious violation of the rights of a country that was not a party to the war, though Vietnamese troops were in Cambodia. The last several weeks of the school term were lost to rallies, marches, and organizational meetings. Students in the school, in line with our training, developed a clearinghouse of information on the war. This activity led to my first publication in the field (Bates, 1970).

Altogether, I taught the Introduction to Information Science course three times at Berkeley, in the spring and fall of 1969 and spring of 1970. Because of riots or other disruptions, I was not able to complete the entire ten weeks of the quarter any one of those three times. I was reluctant to cancel classes for the sake of those students who wanted to continue, but, for a variety of reasons, it was sometimes just not possible to hold a class. In the spring of 1970, the university cancelled the last several weeks of classes for safety reasons.

Other Influences

Another major influence during my years at Berkeley was William Paisley. He was a professor in the Communication Department at Stanford and was invited to teach a course in information needs and uses at the library school. Paisley's original background was social psychology, and his move

into communication research had enabled him to have a broader understanding of and appreciation for the commonalities among normally distinct academic disciplines. He and Edwin Parker were the faculty members at Stanford who were looking at the information aspects of communication. They represented a small salient away from the conventional orientation of communication research toward the study of mass media.

Paisley taught a second course in the user area as well. I have found the two syllabi in my papers. One course was entitled Behavioral Study of Scientific Information Flow, and the other was entitled The Flow of Information to the Public. It is the first of these courses, taught in 1968, that I want to describe in part. The first week introduced "the information systems of science in their historical context." The next four weeks were devoted to "behavioral research methods useful in information studies." Assigned readings for these weeks drew heavily from a classic social sciences methods text, Kerlinger's *Foundations of Behavioral Research* (1964). Each week Paisley took a different broad class of research methods and illustrated it with relevant studies from information science, communication, and related fields. After an introduction to behavioral research methods in week two, he addressed, in succession during the next three weeks, the following: "the logic of nonexperimental descriptive research," "the logic of experimental explanatory research," and "the logic of nonexperimental explanatory research." I quote these exactly to demonstrate the language used at the time. In effect, he was supporting the legitimacy of what is now called quantitative and qualitative research. The terms "experimental," "quasi-experimental," and "nonexperimental" were widely used. I do not recall anyone ever calling such research "quantitative" or "qualitative."

In the second five weeks of the term, Paisley addressed "the information systems of science in their social context," including the "effects of the cultural and political systems on information flow," followed by the "effects of the professional association," "effects of the 'invisible college,'" "effects of the employing organization and work team," and, finally, "information inputs and cognitive processes." The reader may recognize these various contexts from Paisley's 1968 review chapter on information needs and uses in the *Annual Review of Information Science and Technology* (Paisley, 1968). "Information seeking in context" has become a popular byword in modern LIS research and has even generated a separate conference by that name, which began in the 1990s. But such thinking was already well launched thirty years earlier.

It is popular these days to speak of information-seeking behavior research and theory as though it only truly came around to a user-centered orientation in the 1980s. Before then, it is said, user research was system oriented. Paisley used the word "system" repeatedly in his class, as evident from the above, but the scientist is a very real actor in these systems, not a helpless pawn. These are not technological systems but rather human social

systems. Throughout his course and the research he drew upon, there was very much a sense of the scientists both creating and being influenced by these several social contexts.

Many writers, however, cite Dervin and Nilan's 1986 literature review on information seeking as marking, essentially, the beginning of a modern user-centered orientation to information-seeking research. I have long been puzzled at this apparent blanking out of the rich body of information behavior research by Paisley and many other excellent researchers with social science research training prior to 1986. In reviewing Dervin and Nilan's paper, I note that their remit was to review the literature from 1978 forward, as 1978 was the date of the last preceding review of the topic. The following is the first paragraph of a section entitled "Call for a Paradigm Shift" (capitalization of author names was the standard format for the *Annual Review* at the time):

> Since 1978 some scholars have focused their primary efforts on identifying the underlying premises and assumptions that they see as having guided information needs and uses research. They call for developing an alternative set of premises and assumptions—in essence, for the introduction of an alternative paradigm. Notable among these are: BELKIN (1978), BROOKES, DERVIN (1977; 1983b), HAMMARBERG, JÄRVELIN & REPO, LEVITAN, MARKEY, MICK ET AL., NEILL, RUDD, AND THOMAS D. WILSON (1981; 1984). (Dervin & Nilan, 1986, p. 12)

They then go on to summarize what they consider to be the differences between the user orientation represented by the above authors and a systems orientation. Thus, it would appear that subsequent generations of information behavior researchers have read this section and assumed that modern, user-sensitive research on information users began only about 1978 and that Dervin and Nilan were the first to capture this new move in their article. Yet in 1965, Paisley and Parker wrote an article entitled "Information Retrieval as a Receiver-Controlled Communication System" (Paisley & Parker, 1965). Colin Mick, cited above, was a doctoral student of Paisley's.[6] Further, the extensive research cited by Paisley in another, much longer review (1965) and by Herbert Menzel in a 1960 review is overwhelmingly user-centered; these are not studies of information system performance with standardized relevance assessments or collections of studies on library circulation.

I subsequently took User Studies as the second of my two doctoral examination areas for the Ph.D. As the first student to request that area, I typed a giant binder full of notes on all the research of note that I had found to that date and wrote a sixty-page literature review of the essential studies, in effect, to educate the faculty. I subsequently submitted the review to ERIC (1971). I should have developed the work into a book, but I lacked the confidence at the time. I now very much wish I had done so, because it

appears that much of our field currently thinks that user-centered research began in 1978!

I subsequently founded the first courses in information-seeking behavior at the University of Maryland, the University of Washington, and UCLA, in, respectively, about 1975, 1977, and 1981. I have always tried to teach the full historical arc of information-seeking research rather than only the latest work. In ignoring that earlier literature, we collectively have failed to benefit from a rich body of findings that were often based on top-quality research designs that were supported by abundant funding. The 1960s were a golden era where federal social science research funding was concerned; we have not seen the equal since. These studies were not all soulless statistical monstrosities, as so often caricatured in the current world of qualitative research theory. For example, the thirteen information seeking studies that appear in the 1959 *International Conference on Scientific Information,* which I studied closely as a doctoral student, employ a wide range of methods, most quite sensitive to a user perspective. Indeed, Menzel's research (1959) in that volume on the ways scientists serendipitously discovered new information of value to them could be reported today in a modern journal as a qualitative study, and, except for changes in the technologies used, the results are still of value—because *people and the social system of science* change much more slowly than does the technology.

My 1970 Information Science course (discussed above) lists the "User in the System" as a topic because I was a doctoral student attempting to bring a user orientation to a course that had been entirely devoted to a systems approach. I felt I had to relate this interest of mine to the main content of the course, thus I entitled the section "User in the System." In the meantime, however, I had already taken Paisley's and other courses that drew me to a social science research paradigm and an interest in both information seekers and system design. I was particularly interested in access vocabularies that were oriented to users and designed from their needs.

When I took Paisley's course, I was absolutely fascinated. I went to see him in his office one day. We talked for forty-five minutes, and it felt like coming home. There was an intellectual "just-right-ness" about how he thought about things and what his interests were. I had found my preferred intellectual style and content, and, ultimately, my mentor. (He could not formally be my advisor because he was at Stanford.) I was to learn that he had tremendous personal and professional integrity as well. He took me absolutely seriously as a researcher in training—something that was not always easy to come by for a young woman in the era of miniskirts. I could not have picked a better person to look to for guidance and as a model. (Paisley's wife, Matilda Butler, might have served as a female model; however, she was beautiful and had her life so well organized that I could not imagine ever being like her.)

Not long after that, I left Maron as an advisee. My increasing interest in

social science approaches was not a good match with his mathemetico-logical theoretical orientation. Victor Rosenberg, who had joined the Berkeley faculty in the meantime, became my new advisor. Rosenberg had done an early and important study on information seeking for professional needs (Rosenberg, 1967). That study was one of the best-known sources of evidence for the Principle of Least Effort in information seeking at that time.

While working with Rosenberg, I had the opportunity to attend, as a student "gofer," an exciting conference in Palo Alto on the new technology of online database searching, which was just appearing on the horizon in its most primitive form. I did the bibliography for the book that appeared out of that conference (Walker, 1971) and got to meet a number of leading lights of the field, including Pauline Atherton (now Cochrane), Douglas Engelbart, Margaret Fisher, Robert Katter, Frederick Kilgour, Robert Landau, Davis McCarn, Edwin Parker, Mary Stevens, and Roger Summit, the founder of the DIALOG search service.

Rosenberg and I got along well in the advisor relationship, but he left for the University of Michigan before my work was completed. So I switched, finally, to Ray Swank, the dean of the library school, whose interests were less well linked to mine but were sufficiently close to complete the dissertation. Swank was a thoughtful and supportive advisor. My dissertation was entitled "Factors Affecting Subject Catalog Search Success," a topic that nicely melded my two interests in intellectual access and users (Bates, 1972; published in Bates, 1977a and b).

Another influential course was a seminar taught in the library school by James Dolby. Dolby was a professor of mathematics at San Jose State University and was working at that time with Harold Resnikoff on a grant to study information storage and access, especially in library catalogs (Resnikoff & Dolby, 1972). Dolby's course was immensely important to me in ways I did not fully recognize at the time. The seemingly disparate topics he raised and discussed in the class all had in common a deep understanding on his part of the ways in which we can think about information independently of content and still discover wonderful and valuable things about it. It is popular nowadays to be somewhat dismissive of the fascination with information that characterized the 1950s and 1960s. The work from that time is often viewed as reflecting a naïve assumption that information is an objective entity to be transferred from a sender to a recipient and has an identical meaning to both parties in the transaction (Dervin, 1983, Tuominen et al., 2002). I believe that this view misreads how sophisticated at least some of the writers were at that time. But, more importantly, this view also fails to see the positive benefits that arose, and can still arise, from the study of information as an entity distinct from its meaning content. Information can be an indicator of social processes, and it can be considered as a phenomenon of interest in and of itself in a variety of senses.

The final influential course to be mentioned was a seminar given by

Patrick Wilson. I took it in my first quarter in the doctoral program, in the spring of 1967, when Wilson was in the final throes of writing his first book, *Two Kinds of Power: An Essay on Bibliographical Control* (1968). Wilson was trained as a philosopher, and he brought philosophical rigor to the discussion in class. We essentially worked through the ideas in his book during the course of the quarter. The class taught me that our discipline can be as intellectually demanding and as exciting as any body of thought. Though I had had a couple of philosophy courses in college, I had not understood the game of philosophy as it is played by its theoreticians. Wilson's course piqued my interest in philosophy in a more sophisticated sense. Some ten years later, while teaching at the University of Washington, I audited a couple of philosophy courses and frequently went to philosophy colloquia. This background has enriched my understanding ever since.

During this entire time as a student I was also taking reading courses and reading on my own, always with a sense of exploration in a new world and in an effort to pull together a coherent sense for myself of what information science was and where it could go. There had not been many books written within the field yet, but these four formed my understanding of the then existing core: Joseph Becker and Robert Hayes's *Information Storage and Retrieval* (1963), F. W. Lancaster's *Information Retrieval Systems* (1968), Charles Meadow's *The Analysis of Information Systems* (1967), and Manfred Kochen's collection, *The Growth of Knowledge* (1967). For me, Becker and Hayes's work was the canonical description of information science as it began in the 1960s. The book was a rare mixture of the key elements of a science named for information: it covered the management, both physical and intellectual, of information, the structure of retrieval systems, and the theoretical background. Lancaster's book provided a very insightful conceptualization of indexing theory. Meadow's book, though subtitled "A Programmer's Introduction to Information Retrieval," was useful to me because it presented a database management perspective. On the contents page of Kochen's book, there are checkmarks indicating that I had read over half the articles in it, but the article that influenced me the most by far was Derek de Solla Price's "Networks of Scientific Papers" (1967). Along with Price's two short books on the bibliometrics of scientific communication (1961, 1963), this work demonstrated how powerfully the seemingly trivial barebones statistics of information could tell stories of great interest from a sociological and historical perspective. I also read Thomas Kuhn's (1964) and James Watson's (1968) books, both of which, in different ways, shattered some standard assumptions about the way science works, and, implicitly, how science information flows.

Because of the newness of the subdiscipline of information-seeking behavior, there were few books on it, hence my extensive use, as noted above, of my own and others' literature reviews to identify a wide range of partially or wholly relevant resources. As for sources outside of information science,

I read Shannon and Weaver's popularization of Shannon's information theory (1963), which was an important part of Maron's courses. In addition, as the theory drew on mathematics, engineering, and physics, which I found difficult, I also gave close attention to expositions on information and communication theory by Colin Cherry (1966), J. R. Pierce (1961), and Jagjit Singh (1966). I was particularly fascinated with Norbert Wiener and his book on cybernetics (1961). I also read his autobiography, *I Am a Mathematician* (1956), which was rooted in a classic child-prodigy tale. Though cybernetic theory itself has been marginalized subsequently, the core idea that some systems are governed by information fed back from the environment was a breakthrough of enormous significance at the time. We use terms like "feedback" so casually today that we do not realize how fundamentally such ideas shook up science and human understanding generally in the 1940s and 1950s. Wiener's work reinforced for me the idea that there is great power in understanding the role of information at a systems level. After I had held Wiener as a hero for many years, I worked briefly on a consulting job with Joseph Becker (co-author of the abovementioned book by Becker & Hayes) in 1989. He told me that he had met Wiener and that Wiener had dismissed our field as "sorting things into jam pots." So much for hero worship.

More harmonious with my native abilities and cognitive style were materials I read in psychology and linguistics. The psycholinguistics course I took was taught by Dan Slobin, and it represented the cutting edge work of the day. We read Noam Chomsky's brilliant dissection (1959) of B. F. Skinner's book, *Verbal Behavior* (1957). Chomsky's review was one of several forces propelling a movement to restore the validity of studying the mind to the discipline of psychology, in contrast to the mandate to study only observable behavior, which had been the position of the behaviorist paradigm of Skinner and others. In a bibliometric study I did in 1980 of our field, covering somewhat earlier literature, I found that Chomsky was the most-cited person in our field at the time (Bates, 1980). Books such as George Miller's *Language and Communication* (1951), which analyzed language from the standpoint of Shannon's information theory, and Miller, Galanter, and Pribram's *Plans and the Structure of Behavior* (1960), informed my thinking and reinforced the value of understanding life from its pattern and structure, from its information, in addition to its meaning.

Final of influences were the guest speakers in classes or in the regular colloquia that were held in the school or in other departments. I kept notes on these talks. Apart from faculty in the school, such as William Cooper and J. Periam Danton, the speakers whose talks I attended included (in no particular order): Robert Hayes, Paul Wasserman, Lotfi Zadeh, John Bennett, Robert Sommer, Robert Katter, Donald Kraft, Carlos Cuadra, Warren McCulloch, and Michael Lesk. These speakers represented a mixture of the social, engineering, and information sciences. The one woman speaker

I recall and have notes on was Christine Montgomery. She presented a creditable, professional talk on "Content Representation and Information Processing." It was a novel sensation to hear a speaker with whom I could identify more directly than I was accustomed to with the male speakers.

Through all these various influences, I developed a sense of information science as being, actually, about information. For a long time, I took my own understanding for granted, as representing a general way of thinking about the subject in the field. Finally, however, as more and more new influences entered the field, many of them powerful and interesting as well, it seemed more and more important to try to articulate just how our discipline can carve out its own particular territory among the many disciplines competing for some of the same intellectual turf. In a 1987 conference paper, entitled "Information: The Last Variable," I argued for more attention to the discovery of the variables that are unique to the study of information. In 1999, in "The Invisible Substrate of Information Science," I presented much more extensively my view of what uniquely distinguishes information science from other disciplines. See, especially, the section titled "Information Science Theory" in that article. These ideas, developed over thirty years in the field, had their roots in my experiences at U.C. Berkeley in the 1960s.

Discussion and Conclusions

This article began with a question regarding the role of the history of documentation in the development of information science at the University of California at Berkeley. I have reviewed a wide range of influences that chiefly formed my thinking as a doctoral student in the school at the time. These influences are all about scientific, engineering, logical, social, and psychological thinking that formed early thinking in information science as we experienced it at U.C. Berkeley rather than about documentation. It is fitting at this point, however, to refer to the tiny role that documentation did play in my studies there.

When I arrived at the library school, there was still a course on the books titled Documentation, and my recollection is that it was Dean Swank whom I asked about it. He said that it had not been given in several years, and the subject had been a precursor to information science. As far as I am aware, the course was not given again. In reviewing the notes from my schooling, however, I found a lecture on documentation. In a course numbered Librarianship 212A—unfortunately with no title, but I recall it as a course in advanced reference sources—taught by Ray Held in the winter quarter of 1967, the first lecture of the course was on documentation. Perhaps Held had taught it previously? In the notes I took on that lecture, I wrote that documentation largely overlaps librarianship but has slightly different concerns. Documentalists were said to be more interested than librarians were in dissemination; were more likely to focus on new systems, theories,

and technologies; and worked most often in science and technology disciplines. This description sounds a lot like the work of special librarians. In that lecture, I got no sense, or at least retained no sense, of the long history behind the idea of documentation. For me, at Berkeley, information science was something new under the sun, drawing on theory and research from a number of fields, none of them being documentation.

It is no doubt information science's loss that we did not develop a better linkage with the larger theoretical history of our field while students at Berkeley. At the same time, a wide range of deeply developed thinking in the social and engineering sciences *did* enormously enrich our understanding. In the recent enthusiasm for reconnecting with the earlier history of our field, it seems to me that the middle-term history, that of the 1950s and 1960s that I have described herein, is being rather ignored, and the full richness of understanding that is available to our field thus is not integrated.

Of the nine of us who went into academia from the Berkeley doctoral group listed earlier, all but two have taught mainly in the West. Thus the vision of information science developed at Berkeley may not have penetrated much beyond the Rocky Mountains. Considering the standing today of the subject matter that we covered, the subfield of information retrieval has certainly thrived subsequently. Gerard Salton's work at Cornell University in New York, however, surely had a great deal to do with the subsequent success of that subfield as well. Patrick Wilson's sophisticated philosophical analyses of access and information seeking in his three books (1968, 1977, 1983), as well as in the book written by Howard White, Wilson, and myself in 1992, and Paisley's legacy in information seeking (see also Paisley, 1980; Paisley & Parker, 1965, 1968; Parker & Paisley, 1966; Rees & Paisley, 1968, among others),[7] seem to have been much less recognized subsequently—much to the loss of the field, I believe. Whatever the reasons, perhaps now with publications such as this issue of *Library Trends* we are at last developing a sufficient sense of ourselves as a discipline to bring together all of the rich sources from which we draw and to create an intellectual edifice worthy of the exciting questions we study.

Notes

1. Currently enrolled doctoral students are not now permitted to teach graduate courses as sole instructor in their own department in the University of California; presumably, the rules were different then.
2. "The Scientific Method" was put into quotation marks because there are many such methods. The purpose of this section was to provide a simplified, general conception of how scientific research is conducted.
3. These were not what are currently known as computer games; rather, we addressed established nonautomated games with known rules for play. As computer processing power was limited, software had to be based on strategic heuristics rather than on brute force computation of all options, and there was much interest at the time in such heuristics.
4. Wherever possible in this article, the cited book editions are the ones that I would have seen at the time rather than the latest edition available now.

5. I heard later that there is some dispute between the Los Angeles and San Francisco chapters regarding which chapter was actually founded first.
6. Three other advisees of Paisley's in the Communication Department at Stanford have been influential in information studies: Christine L. Borgman, Donald O. Case, and Ronald E. Rice. Case has recently published a comprehensive book on information seeking behavior (Case, 2002).
7. Both Paisley and Parker subsequently left Stanford to establish their own information and communication industry businesses.

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